Model−based Evaluation of Salmon Rearing in Tributary, Mainstem, and Delta Habitats

Steven F. Railsback

Final Selection Panel Review

Proposal Title

#0169: Model−based Evaluation of Salmon Rearing in Tributary, Mainstem, and Delta Habitats

Funding:

Fund with future funds
Amount: **$535,298**

The final Selection Panel agreed with its original recommendation on the merits of this proposal. Due to the recent reduction in funds available for the Science Program's 2004 PSP, the Selection Panel has been forced to place this proposal in the Fund with Future Funds category. This decision was based solely on the current programmatic priorities of CALFED and the current level of available funds for purposes of supporting research efforts of this nature. This decision was not a reflection of the technical merit of this proposal.

Public Comments

No public comments were received for this proposal.

Initial Selection Panel Review

Proposal Title

#0169: Model−based Evaluation of Salmon Rearing in Tributary, Mainstem, and Delta Habitats

Funding:

Fund
Amount: **$535,298**

Initial Selection Panel (Primary) Review

Topic Areas

- Life Cycle Models And Population Biology Of Key Species
- Environmental Influences On Key Species And Ecosystems
- Relative Stresses On Key Fish Species
- Implications Of Future Change On Regional Hydrology, Water Operations, And Environmental Processes
- Assessment And Monitoring
- Salmonid−related Projects

Please describe the relevance and strategic importance of this proposal in the context of this PSP. How does the proposal address the topic areas identified above? What are the broader CALFED Goals this proposal may meet that are not accounted for in these specific topic areas?

This proposal will attempt to meet all of the Science PSP priority topic areas (e.g., water operations, ecological processes and performance assesment) by using an established ecological model and quantitative data to assess CALFED actions on four key species (i.e., fall,late−fall,spring and winter−run Chinook salmon). In addition, the model is applicable to additional research needs such as future restoration actions, climate change, and hatchery vs wild fish interactions. This study directly addresses a key CALFED management issue: what habitat types are most important for

salmonids and what benefits can be expected from restoration actions. The goal of the study is to evaluate and compare salmon rearing success in four major ecological areas of importance to CALFED: small and large tributaries, mainstem, and delta. This information can then be used to determine where best to focus CALFED enhancement efforts. The project will enhance our understanding of life cycle models by examining population changes in response to multiple stressors and how these vary over time. Reliablity of future CALFED actions will be improved by building knowledge of links between environmental processes and key species. The model will attempt to predict what factors (e.g., predation, flow, temperature) influence current escapement trends. Broader CALFED goals met by this proposal are how salmon population biology is coupled to changes in habitats and how shifts in regional hydrology or climate changes will alter those populations.

The budgets of proposals submitted in response to this PSP are larger, on average, than those submitted to CALFED in previous years. The Science Program is committed to getting as much science per dollar as is reasonably possible. With this commitment in mind, can the proposed budget be streamlined? If so, please recommend and clearly justify a new budget total in the space provided.

The budget is reasonable for a 3−yr study of this caliber and design. There could be some streamlining by reducing the number of informational meetings to Sacramento from 4 to 3 trips (one/year). In addition, the cost of overnight lodging could be reduced from $80/night to $60/night(govt. rate). If this were done the budget could be reduced by $7,300, for a new total of $527,998.

Evaluation Summary And Rating.

Provide a brief explanation of your summary rating and any additional comments you feel are pertinent.

In summary, I have little doubt that this proposal will furthur the science needs of CALFED and fill−in much needed knowledge gaps in salmonid life history cycles. The combining of existing 2−D modeling with quantitative data from field

sites is a very cost−effective approach to producing answers quickly. The proposed individual based model is supported by the USEPA as a multiple−stressor assessement model and is considered a step above previously used models in the Central Valley (e.g., SALMOD and PHABSIM). Management of water operations in the Central Valley should be able to use the results of this study to determine which habitat types have the greatest potential for producing salmon smolts and adjust actions accordingly. The study includes a review of available information and an analysis of model sensitivity and uncertainity. Some minor modifications of the model may be needed in order to fit delta habitat paramenters and fish growth in tidal areas. I strongly recommend this proposal for funding based on the information gained and the high marks given by the Technical Review team.

Selection Panel (Discussion) Review

fund this amount: $**535,298**
note:
fund

The panel felt this modeling effort would fill legitimate and important knowledge−gaps regarding salmon migration through the Sacramento River and San Francisco Estuary. For example, the outcomes of this project could be incorporated into real−time freshwater flow management and scenario−analysis.

The project team is composed of people with great expertise in fluvial systems; however, the panel expressed some concern that the team lacks some expertise in brackish, tidally influenced systems. Therefore, the panel strongly encouraged the proponents to collaborate with experts in Delta hydrodynamics and fish response to these hydrodynamics.

In addition, the panel was not clear whether the proposed model could incorporate floodplain habitats in addition to those that are explicitly mentioned in the proposal. The panel felt that floodplain habitats should be included in the modeling effort if that is technically possible.

Panel Ranking: FUND

Technical Synthesis Panel Review

Proposal Title

#0169: Model−based Evaluation of Salmon Rearing in Tributary, Mainstem, and Delta Habitats

Final Panel Rating
superior

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The overall goals of this proposal are to develop a model to compare importance for salmon rearing in 4 major habitat types (small trib, large trib, mainstem, delta), and identify how rearing success in these habitat types is influenced by human activity. The authors would develop an individual based model of fall and spring run chinook salmon, and run simulations comparing fry and parr abundance, survival and growth in these different habitat types. The model would be used to address specific questions regarding restoration and enhancement potential of the different habitat types, such as "How do habitat, biological conditions and behavior interact to affect salmon growth and survival?, How does habitat value change seasonally? Which habitats offer good opportunities for enhancing salmon smolt production?" Specific objectives are to develop, calibrate and verify and IBM with field data, and use this model to run policy scenarios. The approach is justified because,where data are available, modeling can produce answers more quickly and cost−effectively than large field studies, and determine importance of single factors that in nature, are often confounded with multiple other factors. This was a clearly written proposal that demonstrated the care with which the authors will conduct and evaluate their model. They have good knowledge of data availability and quality. After

consulting with local agencies, and some survey work, they will select sites that represent the habitats they propose to simulate. Then, the authors propose detailed field studies to obtain measures of input parameters, such as predation risk and food availability, temperature and hydrologic conditions. The emphasis placed on obtaining careful measurements of model input parameters is reflected in the budget; half of budget was allocated here. THe authors will explicitly consider uncertainty in model parameter estimates. Products will be useful to management, particularly with respect to flow management; management likely has little control over temperature. Reviewers recommended the authors emphasize comparison of model predictions with field data. One reviewer commented that this proposal will not deterimine the importance of habitat types, rather will compare salmon performance (growth, survival and abundance) in each habitat type. Budget is reasonable. THe research team is very experienced, and has been working on studying salmonid habitat issues for several years. They are very familiar with quantifying and simulating foraging and predator avoidance behavior of young salmonids.

Additional Comments:

The overall goals of this proposal are to develop a model to compare importance for salmon rearing in 4 major habitat types (small trib, large trib, mainstem, delta), and identify how rearing success in these habitat types is influenced by human activity. The authors would develop an individual based model of fall and spring run chinook salmon, and run simulations comparing fry and parr abundance, survival and growth in these different habitat types. The model would be used to address specific questions regarding restoration and enhancement potential of the different habitat types, such as "How do habitat, biological conditions and behavior interact to affect salmon growth and survival?, How does habitat value change seasonally? Which habitats offer good opportunities for enhancing salmon smolt production?" Specific objectives are to develop, calibrate and verify and IBM with field data, and use this model to run policy scenarios. The approach is justified

because,where data are available, modeling can produce answers more quickly and cost−effectively than large field studies, and determine importance of single factors that in nature, are often confounded with multiple other factors. This was a clearly written proposal that demonstrated the care with which the authors will conduct and evaluate their model. They have good knowledge of data availability and quality. After consulting with local agencies, and some survey work, they will select sites that represent the habitats they propose to simulate. Then, the authors propose detailed field studies to obtain measures of input parameters, such as predation risk and food availability, temperature and hydrologic conditions. The emphasis placed on obtaining careful measurements of model input parameters is reflected in the budget; half of budget was allocated here. THe authors will explicitly consider uncertainty in model parameter estimates. Products will be useful to management, particularly with respect to flow management; management likely has little control over temperature. Reviewers recommended the authors emphasize comparison of model predictions with field data. One reviewer commented that this proposal will not deterimine the importance of habitat types, rather will compare salmon performance (growth, survival and abundance) in each habitat type. Budget is reasonable. THe research team is very experienced, and has been working on studying salmonid habitat issues for several years. They are very familiar with quantifying and simulating foraging and predator avoidance behavior of young salmonids.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Model−Based Evaluation of Salmon Rearing in Tributary, Mainstem, and Delta Habitats

The researchers propose to develop an individual−based model for Chinook salmon. They propose to use the inSTREAM model. The panel considered this an excellent proposal and considered the proposed work very valuable. A particular strength identified by the panel was that the researchers are proposing

to collect extensive data from these habitats to estimate model parameters and configure the model. They also will estimate the uncertainty in their parameters. The panel felt that the proposal was very well−written.

The only shortcoming that was identified was that the researchers did not propose to compare predictions against observed data, but this was not considered necessary at this time by the panel.

Rating: superior

Technical Review #1

proposal title: Model−based Evaluation of Salmon Rearing in Tributary, Mainstem, and Delta Habitats

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals and objectives of this proposal are quite clear: to evalute rearing success in alternative habitats, and to identify how rearing success in these habitat types are influenced by human activity. Specific objectives revolve around a further developing and calibrating an individual−based model with field data, and using this model to run policy scenarios (among other tasks).**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Overall, the work that is proposed is well justified. There are at least two major issues that need justification, and the authors tackled both of those. The first is the rationale for adopting a modeling approach. The authors argue that direct field measurements of rearing success are expensive and potentially confounded by sampling artifacts. Second, they argue that an individual−based modeling approach is**

	appropriate because (1) this type of model explicitely captures the processes that give rise to varition in rearing success, (2) because of this, direct measures of survival and growth are not needed, because they emerge from the model based on more readily measurable properties and (3) daily variation in environmental conditions and variation among individuals are important in dictating overall average reading success. **For the most part, I am in agreement with the authors on the first point: when there are already extensive data sets available, modeling can produce answers much more quickly (and more cost−effectively) than large field studies. That said, I do think the authors are a bit overconfident in the model predictions. The model output should best be considered hypotheses for further study (in fact, the authors make this same point later in the proposal).** **I am generally more skeptical about the general importance of considering individual−scale variation and daily variation in identifying useful advice to decision makers. Imagine if all of our climate models explicitly kept track of each water molecule! That said, the authors have demonstrated a strong track record of using these models sensibly. This gives me confidence that useful products will emerge from this exercise.**
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to

generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**This project will combine a field component (measuring food availability, predation risk, as well as hydrologic conditions) with a well−thought out modeling study. They authors are skilled in using these models in a way that can identify important uncertainties, as well as generating advice that is robust to uncertainty. Moreover, the entire study is developed around some clearly defined management objectives (e.g., improve rearing success) based on processes that humans can actually control.** **My only criticism is that the model analysis will be based on evaluating only parameter uncertainty, not any structural uncertainty in the model. The latter can be really nasty. Perhaps a class of fish foraging rules could be implemented, rather than specifying a single one (which is essentially a dynamic programming model, right?).**
Rating	**excellent**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**I have little doubt that this work will be feasible. The authors have specified precisely where parameter estimates will be derived, and how the field component will be folded into the model developement. Moreover, analysis of uncertainty is explictly included as part of the model evaluation.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**N/A**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**I anticipate several publications in academic literature, as well as useful products for decision analyses in this system. These authors have a good track record here, so there's very little risk as far as I can tell.**
Rating	**excellent**

Additional Comments

Comments	**This is one of the best written proposals I have read in a long time. I hope you choose to support this activity.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**Exceptional (see comments above).**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**For the most part, most of the budget is directed towards salaries and field support. Salaries for the two field crew leaders are a bit steep, but that's the price you pay for competent help. I did note that the lodging rate ($80 per person per day) for the field work was pretty steep. Seems like this could be cut in half very easily.**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**I strongly recommend this proposal for funding. This was an exceptionally well written proposal that I enjoyed reading. The authors and I have somewhat different modeling philosophies, but they made convincing arguments to support their approach. Given the strong track record of the PI's, I see can't see any drawbacks to supporting this work.**
Rating	**excellent**

Technical Review #2

proposal title: Model−based Evaluation of Salmon Rearing in Tributary, Mainstem, and Delta Habitats

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals, objectives, and hypotheses are clearly stated and internally consistent. The ideas are timely and important, but I am concerned that model validation and testing is not given enough attention.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study is justified relative to existing knowledge. The conceptual model is clearly stated and explains the underlying basis for the proposed work. The selection of research is well−justified.**
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach is well−designed and appropriate. The results are likely to add to the knowledge base. It is likely to generate novel methods and approaches. The information will be useful to decision−makers if it can be validated with field data.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is well documented and technically feasible. Its likelihood of success is high. The scale is consistent with objectives and well within the grasp of the authors.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The monitoring is well−designed, and there are well−defined plans to interpret the monitoring data. Again, I am concerned that model validation – comparison of monitoring results to model predictions – does not receive enough attention.**
Rating	**very good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The products will be valuable for management decisions if more attention is given to model validation. Contributions to larger data management systems are relevant and well−considered. Interpretable outcomes are likely and well documented.**
Rating	**very good**

Additional Comments

Comments	**None**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The authors have excellent track records. The project team is well qualified to implement the project. They have infrastructure and support needed to complete the project.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is reasonable and adequate.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**Most all aspects of the project are well though out. My only reservation is that without more extensive**

	validation – comparison of model predictions to field data on habitat use, growth, and survival by chinook – it will be difficult to know whether or not model results correspond to real−world behavior of the subject species.
Rating	**very good**

Technical Review #3

proposal title: Model−based Evaluation of Salmon Rearing in Tributary, Mainstem, and Delta Habitats

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**Yes, these are well articulated and developed with increasing detail as the proposal is generally well articulated. I challenge the philisophical statement that the "importance" of different kinds of habitats are being evaluated. To my read, it seems that the habitats will be compared with this rather abstract measure. The habitats themselves are variable in time and the needs of the salmon vary with life−history stage.** **This study seems well posed to answer questions about the local circumstances that make a given habitat better or worse than another set of conditions, and how these change over time. These are the first two "proposed study questions" they plan to address, but the third questions of how habitat types offer opportunities for enhancing salmon production is inseparable from the life−history of the stocks and the temporal pattern of habitat use, priority, sequence etc. that may be beyond the scope envisioned here.** **The idea of "Importance" carries a lot of baggage with it: for which fish? and when? and under what circumstances? and does this change with environmental conditions, age, etc. An "it depends" result is already known.**
Rating	

	good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**I actually believe that the proposal has sufficient merit based on only the first two of the proposed study questions. It is justified for the comparison and analysis of how each habitat type functions to create a rearing environment that can be used by salmon and under what circumstances management actions could improve or damage this functionality.** **It will be a good extension of the IBM model that has been developed and will examine the delicate dynamics of predator, prey, and environmental conditions.** **Well conceived models provide great environments for conducting "experiments" that are impossible to accomplish otherwise.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The immediate applicability of the project will depend on whether significant environmental or biological variables are within the control of river managers. Perhaps flow will be controlled easily but not so temperature. They do recognize differences in the influences of**

	environmental conditions and the differences that various management scenarios could in turn impose. **Within the habitat types proposed, the best course of action may be quite different and that would help target dollars to where and how they can be best spent.** **The relative importance of one habitat over the other will be more difficult to ascertain.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The modelling effort is certainly feasible and within the grasp of the authors. they have applied their model in the past to similar circumstances. The most significant task is the collection of the local environmental data at each site, and they have allocated nearly half their budget for this purpose.** **I am a little uncertain about the applicability fo predation risk assessments based on tethered fish to a broader context, however. Perhaps this si the way to calibrate such a parameter. It will be worthy of close scrutiny and detailed reporting independent of the larger context to which it is being applied.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**N/A**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Yes products will be valuable. I am not sure how the data will be made available to others involved in similar research, although a plan is in place for reporting, analysis and presentation via publications, presentations, etc.** **I completely expect to learn that specific environmental conditions have specific impacts on the salmon in the habitats being studied. More useful will be an understanding of the ranges of conditions that yield acceptable results and an analysis of the likelihood that conditions can be kept within those ranges and under what circumstances. Of particular note will be the application of climate change scenarios to regional management questions. It is entirely possible that once ideal habitats are no longer so after temperatures are altered significantly. Anticipating this can help screen sites for restoration/protection with a long−time−frame perspective.**
Rating	**excellent**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**Their track record is great and the team is likely more qualified than any to integrate this model with field data. The model they propose to use is becoming more and more established in management (according to their statements) and they have a diverse group providing support.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Actually seems like good value for some things. Perhaps because they can fund employees of agencies and institutions instead of hiring specific task consultant and can clearly articulate their data collection needs.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**It will be especially interesting how they will detail the "importance" of different habitats when they also admit that "juveniles have a diversity of life histories and rear in a variety of habitats." This applies to different runs as well as individuals and that individuals can plausibly "choose" among the habitats. In the very least, how could a fish in a tributary NOT pass (and perhaps use in a significant manner) the mainstem and the delta en−route to the**

	ocean? **If this group can calibrate and use this model to reach all of the goals they aspire to, it will be worth the expense as it could lead to some very focused management changes. In addition, understanding the predation risk vs. growth trade−offs for these fish will have applicability beyond the immediate needs of the Science Program.**
Rating	**excellent**